doctrine is put by Prof. Sachs is specially applicable to plants, in which the continuity of the germ-plasm can only be traced through the embryonic substance of the growing points.

This collection, containing what are probably the most important contributions of our time to the physiology of plants, is in itself a marvellous record of scientific activity. As Prof. Sachs is still engaged in physiological research, we may hope that a later edition of the work will contain many further invaluable additions to our knowledge.

D. H. S.

OUR BOOK SHELF.

On English Lagoons. By P. H. Emerson. (London: David Nutt, 1893.)

YET another book about the Norfolk Broads, or, as the author prefers to call them, the "English Lagoons." One can hardly credit that anything fresh could be said on this well-worn subject, but Mr. Emerson's book differs from all that have gone before in being a continuous narrative of a twelve months' sojourn on the Broads in his pleasure wherry, the *Maid of the Mist*, and presents to us a graphic picture of these waters under their winter aspect as well as under a summer sky. Much that he has written, more particularly his excellent descriptions of the peculiar scenery of this remarkable admixture of land and water in mid-winter, is highly interesting. The atmospheric effects under various conditions of storm and sunshine, by moonlight and at early dawn, display a keen artistic perception, but the incidents as a rule are trivial in the extreme in fact, and the constant use of the vernacular becomes tiring—whole chapters (*e.g.* Chapter xxi. of six pages) might have been well omitted.

From a naturalist's point of view the reader cannot fail to be pleased with the kindly spirit which pervades the book, the evident delight which the author took in his feathered friends, and his disgust for the wanton destruction which is too frequently committed by thoughtless visitors to these delightful retreats, but having said this we confess we are rather puzzled by Mr. Emerson's ornithology. On page 216, for instance, he mentions watching a pair of desert wheatears on Palling Sand Hills; surely he cannot have met with *Saxicola deserti* in Norfolk. Scarcely less astonishing is the mention of a blue-headed wagtail's nest, and the appearance of the white wagtail on several occasions. The present writer has known the Broads for forty years, but has never had the good fortune to meet with *Motacilla flava* or *M. alba*, both of which are excessively rare in Norfolk, and probably only occasionally appear as passing spring migrants. Many of the observations on birds are interesting, but the following passage is hardly in good taste. Speaking of Surlingham Broad, "which the late Mr. Stevenson, the local naturalist, loved," Mr. Emerson continues, "But this piece of water is to me dull and songless, but then Mr. Stevenson did not know shadows from reflections, nor, I suspect, beauties from commonplaces. As a naturalist, moreover, he was not to be compared to the late Mr. Booth, a true lover of birds and of outdoor life. But in Norfolk every native goose is a swan." Mr. Stevenson's reputation as an ornithologist is too well established to need any defence from my pen, but I can say without hesitation that the best general description of the Broad district ever written is to be found in the introduction to his "Birds of Norfolk," and his chapters descriptive of a summer's night and a summer's day on the very Broad which Mr. Emerson considers so uninteresting, show not only his wonderful powers of observation but his keen perception of the beauty and poetry of nature; even so familiar a bird as the redbreast is invested with fresh interest after reading his charming chapter on this pert little friend of man.

J. S.

The Mechanics of Architecture. By E. Wyndham Tarn, M.A. (Crosby Lockwood and Son, 1892.)

THE modern architect is beginning to perceive that he has allowed the engineer to cover the ground with monstrosities because his immediate predecessors considered that any scientific knowledge would spoil the artistic faculty, regardless of the great architects of the past, Sir Christopher Wren, Leonardo da Vinci, and the designers of our cathedrals.

The theories which the author discusses, as of Pillars, Roofs, Arches, Domes, and Spires, Buttresses and Foundations, are illustrated by numerical applications to well-chosen existing examples; so that the architect will thereby acquire confidence in the formulas, and not lay himself open to disaster in consequence of a mathematical misprint.

Graphical constructions are freely employed, which recommend themselves to the draughtsman, who thinks better on his drawing-board than in symbols and formulas.

G.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Telegony.

THIS is a term which Prof. Weismann has recently coined to designate a class of phenomena which have thus far been pretty generally accepted as of unquestionable occurrence in mammals, if not also in birds. I refer to the alleged influence of a previous sire on the progeny of a subsequent one by the same mother. The most notorious instance of this alleged fact is that of Lord Morton's Arabian chestnut mare, which had her first foal to a quagga. Subsequently she produced two colts by a black Arabian horse. These were both partially dun-coloured, and striped on the legs more plainly than the real hybrid had been. One of the colts was also striped on the neck and some other parts of the body; lastly, the mane in both resembled that of the quagga, being short, stiff, and upright. Darwin, from whom this description is taken, records an almost exactly parallel case, on the authority of Mr. James Weir. He also gives a number of references to other cases, not only in horses, but likewise in sheep, swine, dogs, &c. Within the last twelvemonth another seemingly unmistakable case of the same thing took place in the Zoological Gardens, and is recorded by Mr. Tegetmeier in one of the December numbers of the *Field*. Here the first foal was a hybrid between two species of ass, and the second by a male of the same species as the mother. Not a few further apparently well-authenticated instances might be mentioned, but these are enough for present purposes. Indeed, most breeders and fanciers are so persuaded of the truth of "telegony" as to deem a pedigree animal seriously deteriorated in value if she has been covered by an inferior male, while in Darwin's opinion "there can be no doubt" as to the fact of this influence of a previous sire being occasionally exhibited in mammals, although he expresses himself as doubtful with regard to it in the case of birds.

Prof. Weismann, however, has recently challenged the facts. He has also given his explanation of them, supposing them to be facts. Therefore I will consider these two points separately.

Several years ago I undertook an experimental inquiry upon the subject with dogs, which yielded negative results. I then obtained an introduction to Mr. Everet Millais, in order to profit by his large experience and scientific interest in all matters pertaining to dog-breeding. He suggested that the question ought to be put in the journals of fanciers in this country, and also in America, for the purpose of raising discussions upon it. This was done, with the result of letting loose floods of letters to

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the journals, and not a few private ones to ourselves. Of course this copious response was for the most part valueless, further than to show a general belief among fanciers and breeders in the facts of telegony, coupled, however, with great differences of opinion touching the frequency of its occurrence. Nevertheless, out of all this medley of unscientific assertion, there were a comparatively few cases where it did not appear that coincidence, pre-formed ideas, mal-observation, atavism, &c., could be reasonably assigned, and these served to indicate the most promising varieties with which to work in future experiments.

The general result of our inquiry thus far has been to corroborate the opinion with which we both started, viz. that although the fact of telegony is of very much rarer occurrence than is generally supposed, it nevertheless does appear to take place occasionally, and especially, as Mr. Herbert Spencer has recently observed, where the first offspring has been a hybrid, as distinguished from a mongrel.

On the other hand, there does not seem to be any good evidence of the phenomenon in the case of mankind. For although I have met with an alleged instance of a white woman who, after having borne children to a negro husband, had a second family to a white one, in which some negro characteristics appeared, I have not been able to meet with any corroboration of this instance. I have made inquiries among medical men in the Southern States of America, where in the days of slavery it was frequently the custom that young negresses should bear their first children to their masters, and their subsequent children to negro husbands; but it never seems to have been observed, according to my correspondents, that these subsequent children were other than pure negroes. Such, however, was not the same case as the one above mentioned, but a reciprocal case; and this may have made a difference.

So much, then, for the facts. As regards their interpretation, Mr. Herbert Spencer says, speaking on behalf of the Lamarckians, "And now, in the presence of these facts, what are we to say? Simply that they are fatal to Weismann's hypothesis. They show that there is none of the alleged independence of the reproductive cells; but that the two sets of cells are in close communion. They prove that while the reproductive cells multiply and arrange themselves during the evolution of the embryo, some of their germ-plasm passes into the mass of somatic-cells constituting the parental body, and becomes a permanent component of it. Further, they necessitate the inference that this introduced germ-plasm, everywhere diffused, is some of it included in the reproductive cells, subsequently formed. And if we thus get a demonstration that the somewhat different units of a foreign germ-plasm permeating the organism, permeate also the subsequently-formed reproductive cells, and affect the structures of the individuals arising from them, the implication is that the like happens with those native units which have been made somewhat different by modified functions: there must be a tendency to inheritance of acquired characters." (*Contemporary Review*, March.)

On the other hand, Prof. Weismann says that, even admitting the facts, they in no way militate against his theory of germ-plasm. For, as he says, "such cases could be accounted for from our point of view by supposing that spermatozoa had reached the ovary after the first sexual union had occurred, and had penetrated into certain ova, which were still immature. The immediate fertilisation of the latter is rendered inconceivable by the fact of this immaturity; but the sperm-cell must have remained in the body of the ovum until the maturation of the latter, with the nucleus of which it then united in the process of amphimixis." ("The Germ-Plasm," pp. 385–6.)

It seems to me that we have here, in principle, a sufficient answer to the Lamarckian interpretation of the facts alleged. I say "in principle," because the obvious objection that mammalian spermatozoa cannot be held capable of delving their way through the stroma of an ovary in order to reach unripe ova, may be obviated by supposing that it is the "ids" and "determinants" of disintegrated spermatozoa which do so. For, if there are any such things as ids and determinants, it is certain (from the facts of atavism) that they can survive the disintegration of their containing spermatozoon, and also that they can then penetrate somatic tissues to any extent.

But I have discussed the whole subject in a lengthy appendix to my recently published "Examination of Weismannism," to which I must refer for all details, both as regards the alleged facts and their rival interpretations, My object in raising the issues in these columns is to ascertain whether further light can be thrown upon the subject by any of your numerous readers. Therefore I will merely add that numerous experiments which during the last eighteen months I have been conducting with birds, have yielded uniformly negative results. Scores of purely bred ducks (white Aylesbury), and dozens of purely bred chickens (Polish) have been hatched; but in no one case has there been the smallest resemblance to their telegonous sires. In some cases a year, and in others only a fortnight was allowed to elapse between the successive impregnations; but in all cases the broods are as purely bred as if their respective mothers had not previously borne offspring to males of widely different breeds.

GEORGE J. ROMANES.

Christ Church, Oxford, September 16.

Quaternions and Vectors.

IN his recent letter (NATURE, August 17, p. 364), which is avowedly a reply to my paper (Proc. R.S.E., 1892–93) on "Recent Innovations in Vector Analysis," Prof. Gibbs does not seem to me to discuss the real point at issue.

At the end of that paper I summarised the arguments in favour of quaternion vector analysis under five heads.

The first of these was: "The quaternion is as fundamental a geometrical conception as any that Prof. Gibbs has named." This argument, which was a direct criticism of Prof. Gibbs's attack on quaternions in his letter to NATURE of two years ago, is not even referred to in his recent letter. It may reasonably be assumed that silence means consent.

The second summarised argument was: "In every vector analysis so far developed, the versorial character of vectors cannot be got rid of." Regarding this, which was a direct criticism of the position of Mr. Heaviside and Prof. Macfarlane, I am glad to find that Prof. Gibbs is virtually at one with me, and brings to my support the great names of Lagrange and Poisson. Now Hamilton's quaternions is admittedly the only vector calculus which takes direct cognisance and makes full consistent use of this principle, the logical consequences of which form the subject of my third and fourth summarised arguments. Thus the quaternion wins all along the line.

The fifth and last summarised argument was: "The invention of new names and new notations has added nothing of importance to what we have already learned from quaternions." This, probably, has most direct connection with Prof. Gibbs's recent letter, which is to a large extent an exposition of his own system. And interesting though this may be in itself, it does not really make out a case against quaternions; and that, be it remembered, is the point at issue. Indeed, Prof. Gibbs himself admits that the quaternion notation has a certain advantage in simplicity. This is plainly so in the case of ∇, of which in its quaternionic form Prof. Gibbs gives a very neat application in an equation whose physical interpretation is the solution of an important problem. But in this very connection, carried away by the exuberance of his humour, he seems to imagine that the *náme* Nabla is of the essence of quaternions, and that the quaternionist has no right to use the word potential.

I am not aware that I anywhere expressed a dislike to the notations $[\phi]$, ϕ_s, $\phi_\times$, which represent quantities most emphatically quaternionic, or at least Hamiltonian, in their origin. What I wished to emphasise was that, in getting at the conception of the quantities ϕ_s, $\phi_\times$, Prof. Gibbs makes use of the so-called indeterminate product, which is no vector but is analytically the same kind of thing as the quaternion product, and that consequently his pamphlet and his first letter to NATURE are hardly consistent with each other.

I am accused of an inadvertence in the interpretation of certain integrals. I have not Prof. Gibbs's pamphlet by me at present, but, if I recollect aright, there is no explicit mention in it of the restriction that the operand is to be a constant vector. Nor do I see that such a restriction is necessarily implied in a system in which operators, whether under an integral sign or not, are represented symbolically apart from the operand. The operand is virtually there all the time. The equations are meaningless without it. To introduce the unexpressed operand is therefore a very different thing from the act of introducing an altogether extraneous vector. With the required restriction, however, it appears that Professor Gibbs's integral operators are not of such general applicability as had been hoped.

But even granting that I have been guilty of an inadvertence on this point, that in no way affects the general argument.

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